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ABSTRACT

Educational research is not having the impact on teaching practice that it could and should be having. In the past fifteen years, a four-tier pattern has emerged, with each tier more complex and revealing than the one before. Earlier researchers focused on description and differentiation in their studies, building a body of knowledge that could be tested and validated. After placing values on the findings, practice is altered, and improvement is sought. However, difficulties have arisen due to the lack of a comprehensive and systematic view of how research findings can and should be used to influence change. Recently, educational researchers have moved toward strengthening the understanding of process-product relationships, making it easier to use and apply research results. Teacher educators should make sure that prospective teachers not only know the terrain of teaching and learning (product), but also know about it (process). In order to make the best use of research findings, teachers must conceive of and support appropriate delivery systems and take research utilization beyond edict, policy, and entreaty. (FG)

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EFFECTIVENESS RESEARCH AND TEACHER EDUCATION: YESTERDAY, TODAY, AND TOMORROW

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Introduction

Despite its Fellini-esque title, this paper has as its purposes conventional ones for a meeting of this kind. They are to (1) speculate about the impact upon teacher education of certain studies of teaching conducted during the past fifteen years, (2) consider certain current or very recent studies of teaching from the same vantage point, and (3) suggest directions for increasing the utility of research on teaching for programs of teacher education. It should be noted that teacher education here is considered to be opportunities intended for teachers in service and teacher candidates to be more effective in schools and classroom settings. Naturally, the distinctions to be made between and among teacher candidates and veteran teachers can be significantly and sharply delineated but for this paper examples are presented which, to me, will not suffer greatly from those distinctions.

Knowledge is Power or Understanding Equals Improvement

As is often the case in a scientific (or pseudo-scientific, as some might argue) endeavor, first steps in inquiring into teaching were descriptive ones. That is, an event was scrutinized from a particular vantage

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point not so much for the purpose of altering it but primarily for the purpose of being able to describe it; knowing what it is. Very often these initial forays into ill-understood areas of human behavior, in this case classrooms and schools, examined minute, molecular phenomena in great detail. As information about the particular began to accumulate, aggregates of particulars were looked at in combination. Still, the intention of the examination was rooted at the descriptive level of understanding. Eventually, the descriptions were themselves examined for patterns, for similarities and differences, for relationships which occur in such a predictable fashion that one could be reasonably assured that certain well-understood phenomena, interacting in certain well-understood ways, would be associated with other phenomena and other interactions.

Such appears to be the case in terms of much of the past two decades' work with research in teaching. In certain long-range inquiries (and by long range I mean at least five or more years), a certain pattern emerged. The pattern might be considered as four-tiered, each tier being more complex and revealing than the one before. The first tier is where I started these remarks, description. A careful and rigorous examination allows us to describe events and activities with reasonable accuracy and efficiency. The second tier, differentiation, allows us to examine these descriptions and draw distinctions between sets of what we have described. The third tier, validation, provides us with sufficient evidence, usually a consequence of repeated tries at understanding, and convinces us that what we have described and the differences we have seen are in a large sense true. That is, we can depend upon the differences holding up,

whether in similar settings or different doesn't matter as long as we are careful to make known what seems to surround our valid phenomena. Fourth, and this is the tricky one, is the tier I call improvement. I mean that we place some values upon what we know about and the distinctions which are clear to us within that knowing and make decisions to alter in a positive direction the differences which we believe to be less than ideal.

As I examine the research on teaching of the past fifteen years, it appears to me that early researchers focussed upon description and, to some degree, differentiation. This impression is underscored by a passage from a 1971 volume edited by Bellack and Westbury in which Westbury wrote:

> The proposition that there is a clear distinction to be made between teaching and learning, as objects of enquiry, has been widely accepted . . . The distinction has defined a subject matter for investigation and has justified, heuristically at least, much of the recent interest in teaching as an activity worth empirical study for its own sake (p. 228).

That comment, it should be noted, was written in response to the proceedings of a 1967 meeting which formed the core of the book.

The studies of teaching which, to me, stand out as exemplars of what I label description and differentiation, but primarily the former, are those conducted by Flanders (1970), Bellack et al. (1965), Hughes (1959) and, although of a different order, Bales (1950). I place Bales in a somewhat separate place here because his work was directed toward understanding interactions in small groups, not necessarily in teaching and learning groups. It must be admitted, however, that much of the classroom interaction work of the late Fifties and early Sixties owes much to

the work of Bales and, indeed, his scheme for examining small group dynamics was applied frequently to teachers and students.

These workers helped us to understand certain interactions between teachers and students in terms of direct and indirect teacher behavior, to move with some understanding through the theretofore complex maze of "the language of the classroom" as a series of participant moves, to consider certain behaviors of teachers as qualitatively different from others, and be able to track the interactions of group members with some precision and accuracy. Some of us will remember the value-laden gasps of astonishment when it was discovered that teachers engaged more often in direct teaching behavior than indirect and that the ratio was far and away in favor of the direct. Let's take an isolated example of how this impacted some programs of teacher education.

Remember, at the time of this revealed truth there was little evidence to suggest that direct was less good than indirect or, in fact, that either made much of a difference in terms of student outcomes, cognitive gain or otherwise. Notwithstanding, I recall several members of a California teacher education faculty discussing these findings with students and among themselves with heavy emphasis placed upon the question, "How can we alter this state of teaching affairs?" Clearly, a value tending toward the indirect methods of instruction was a part of that group's implicit norms. More precisely, those norms were influenced heavily by a pervasive, although somewhat declining, influence from the so-called progressive education era.

These conversations metamorphosed into lengthy sessions in graduate

courses and seminars on how to reduce teacher dominance of the classroom, in undergraduate courses into admonitions to allow students their rightful place in classroom discourse, and in inservice education workshops offered in neighboring school districts into passionate appeals for "teacher as helper" rather than "teacher as teller." Mind, if you will, that I am not arguing against any of these propositions for how teachers ought to behave, given certain assumptions about classroom groups. What this example tells me, though, is that the translation of research into practice in these instances of descriptive studies of classroom interaction was influenced equally if not more by the values of the translators than it was by the original research stimulus. In a strict research sense, these descriptions of classroom life did not suggest that certain patterns of behavior were bad or good or neutral except from a valuing point of view. There was no evidence at this time that indirect was better than direct for promoting self-esteem of students, that certain teacher moves in the classroom game were associated with more-than-predicted cognitive gain, or that certain social interaction patterns in classrooms helped students (or teachers, for that matter) increase their abilities to understand or influence others.

As I see it, the research into practice pattern was set and has continued. Research designed to tell about classroom life somehow became transformed into research which was used to change classroom life. Put another way, the utility of the research was seen by teacher educators as being a great deal more powerful than increasing understanding, of adding to our knowledge base, or as providing a research-derived set of roadmaps

of the classroom -- its utility was given much more impressive credentials. It told us to make changes. And the nature of those changes were less a consequence of a research activity than of a value placed upon the findings of the research.

As I said before, I am not arguing with any force against the place of values and beliefs in making decisions about teaching and about teacher education. But I am concerned that many programs have been promulgated, promoted, and packaged as a consequence of a less-than-comprehensive and systematic view of how research findings can or should be used to influence change.

As an old curriculum type, it's impossible not to draw an analogy from the arena of curriculum planning: specifically, the Tyler (1950) rationale. Although critics have beaten, burned and stomped this rational-empirical way of formulating curricula for decades (most elegantly, perhaps, by Kliebard [1975]), the formula has persisted in practice. Why? I suspect because it assumes a rationality and a logic which is highly seductive. The assumption is that once one has formulated objectives for instruction in precise, behavioral terms, it follows that one can then make sound decisions regarding the most appropriate learning opportunities to accomplish those objectives for and with students. Where does the technology exist to make that enormous leap from ends to means? Where is the store of craft knowledge, accumulated over time and documented all the while, that will give us any surety about the effectiveness of certain instructional modes to accomplish certain curricular objectives? Where is there any large body of evidence that curriculum planners, be they teachers or

publishing house editors or university professors, can make the transition in planning from purpose to learning opportunity with consistent positive outcomes for students? I know of no affirmatory answers to these questions.

Likewise, with the landmark studies of teaching I have cited, I know of no research-derived or research-based evidence which suggested how teachers were to behave to accomplish certain well-defined ends as a consequence of such behavior. What I do know, and from too-many years of experience, is that teacher educators (and, to be fair, a goodly share of the citizens of the United States) have used such studies as stimuli for promoting ends-in-view when the studies themselves offer no safe assurance that there is any particular value in pushing toward their accomplishment. The linear, a to b to c system works. But unfortunately, in too many cases the lacunae in the system are filled in by non-system data, principles and generalizations.

The Current Scene

When speaking of the early studies of teaching in terms of their influence upon teacher education, I suggested that these efforts were principally descriptive and, in varying degrees, confirmed differential patterns of teacher and student behavior. In the past five to ten years, certain research workers have moved beyond the initial confirmations of differences and description to the validation of those descriptions and differences in carefully designed studies meant to ensure that the patterns are persistent and robust rather than artifacts of particular settings. This movement toward strengthening our understanding of certain

process-product relationships is especially evident in the classroom management research arena (Evertson, 1981; Emmer, Evertson, and Anderson, 1980). What we have been privileged to witness is the consequence of persistent efforts to come to some reasoned conclusion regarding the power of certain management techniques as they relate to certain pupil behaviors. This persistence has taken several forms: repeated studies in same and different settings, derivation of principles and generalizations across similar studies, and meta-analyses of studies with the same objects of research attention.

The benefits of such procedures include not only the obvious one of stengthening conclusions regarding certain relations between and among classroom events. There is also the less-then-serendipitous payoff of discovering in a systematic fashion new directions for research into the same phenomena. Again, the research on classroom management provides an example. As the correlations between effective teaching and student cognitive gain began to be verified, attention was shifted in two specific directions. First, and probably most well-known, was the attention which has begun to be given to expanding the understanding of management into curriculum areas and grade levels which were different from the ones used in the first several stages of the descriptive and verification work. That is, do the same principles of classroom governance appear to "work" in dissimilar grade level groups and with markedly different curriculum emphases? However, an equally important shift was the attention given to alternate paradigms for the conduct and analysis of inquiry into classroom processes. We have seen recently a concerted effort by many researchers to move beyond the molecular to the whole. Attention to context (including

of course, grade level of student and curriculum area) has increased our understanding and has deepened our awareness of the complex interactions which quite commonly characterize classroom life (Ward, 1977).

This acknowledgement of the power of the context variables as influencers of participant behavior is moving forward with vigor and appears to be gaining a fair number of proponents despite the longstanding methodological argument over the quantitative versus the qualitative. The argument for "seeing" the context variables in interaction and over time useing naturalistic research procedures seems to have been strong enough to cause some researchers to shift, in varying degrees, their perceptions regarding the relative usefulness and rigor of research techniques adapted from anthropology and sociology when competing with procedures drawn from psychology.

Complementary to this expanded research orientation is the renewed attention to coming to a better understanding of the teacher. Again, members of the research community have made some subtle but, to many of us, radical shifts in both focus and method. Briefly, we have seen a movement away from single-minded attention to a single teacher trait or state variable to an attention which depends upon a more complex view of what a person called a teacher is and does. Recent work by Sprinthall (1979), Kohlberg (1977) and Hunt (1978) encourage us to continue our search for understanding the teacher in his or her various roles -- including those of person and of a member of an occupational group.

What do these few examples mean in terms of the relation between research on teaching and teacher education? In that I believe that a principal weakness in much of the research on teaching, in terms of its usefulness for teacher education, has been the absence of inclusion of

the complexities of classroom settings, I am encouraged by the gradual but increasingly visible attention currently being given to the tangle of classroom relationships and their interaction with context variables. As one concerned with teacher education, I am heartened that the content of teacher education, largely propositional and speculative it must be admitted, can now be informed by a growing body of research evidence which is considerably more reflective of life in schools than that which has preceded it. I am aware, however, of the enormous importance that continued attention to the molecular research view will continue to play. It appears that we are beginning to amass a body of research information and a set of research procedures and paradigms which will provide a substantial part of our knowledge base as we make decisions about teacher education programs.

There is still some concern for how this body of knowledge is to be used by teacher educators. The pattern of research into practice with little attention to appropriateness, harmony, or participant-context match persists. In a recent conversation with a staff development director of a moderately large school system, for example, I commented on the extraordinary effort being expended in making known the principal findings of the Beginning Teacher Evaluation Study (BTES) (Denham and Lieberman, 1980). My friend noted that his district was making it a policy that Academic Learning Time (ALT) be increased in every classroom in the district. He then went on to explain how this procedure would be monitored and how teachers would be informed of the consequences of the monitoring. At no point in our discussion did this person charged with providing

inservice teacher education opportunities discuss the reasons for believing that increased ALT across subject areas and throughout the grades was to be desired. More important to me was the complete absence of any attention to involving the teachers in either informational or change sessions. I would like to believe that this is an isolated example of the pattern of research into practice in teacher education. I fear, however, that it is only one of the most recent examples of that persistent scheme.

Where Might We Go from Here?

The discussion to this point, I hope, has pointed up some of the problems as well as acknowledging some of the progress associated with research on teaching as it relates to teacher education. Quite obviously, to me at least, there are gaps in the research on teaching and, even more obviously, there is a major problem in how we teacher educators deal with research on teaching as we work with our students or colleagues.

Earlier in this paper I noted that research on teaching findings can become content for teacher education. Naturally, how that content is used is dependent upon the purposes of the person or institution making the critical decisions about use. I would like to propose three ways for teacher educators to use research on teaching procedures and findings in their work. (I will refrain from giving any more attention to how not to use research to inform practice.)

First, I believe it essential for teachers in training and teachers in service to have opportunities to know about how classrooms function, how teachers appear to affect students, and how the individual, classroom, school, and community interact. I would hope that we could place into a

comprehensive program different ways of knowing these complex phenomena. One way of knowing, certainly, is from research inquiries such as we have been attending to here. There are, of course, other means to understand -- intuition, prior experience, the setting forth on a journey to discover, and, of course, understanding as a part of a system of beliefs. I would like to see teacher educators, in colleges and universities as well as in school settings, help their clients see the differences such various stances make in understanding and be able to forecast how those differences relate to teaching activity. Although the focus of this paper is upon research, I believe it important and just that competing means of understanding be acknowledged and given attention in teacher education programs.

Naturally, the reasons for giving attention to knowing are many and varied. I will note only two here. They fall into the familiar process-product language game but with a bit of a difference from the way that phrase is typically used in talk about research. I believe it important for teacher candidates and teachers to know about. This is familiar terrain for teachers and for teacher educators. The object of knowing here is product. It manifests itself in answers to such questions as: What is known about the relation of teaching behaviors and student outcomes? of pupil behaviors and peer group interaction? of classroom control mechanisms and child behavior? and so on. Less familiar, particularly for preservice teacher education students, is the process dimension. That is, how did we come to know about these relations? What assumptions guided investigators toward that knowing? What procedures followed from the initial assumptions? How were the outcomes influenced by the procedures?

What limitations must be placed on what is known by the processes through which the knowledge was revealed? It appears to me that persistent attention to the process dimension of knowing will help to alleviate some of the irrationality and inappropriateness of adopting whole either research findings or belief systems. After all, why should the research community dismiss the apparent values in consumer education when it comes to teachers as potential users of research?

A second way I would suggest that the research on teaching body of literature be used for teacher education purposes relates to the earlier mention of the validation aspect of my four-tiered proposal. If teachers are to be considered professional, I believe it important that they engage in systematic inquiry into their own practices and into creating a more detailed and intimate understanding of their workplaces than is usually the case. Typically, teachers are concerned with the dailiness of their school lives (Griffin, 1978). This concern, however, is more often than not demonstrated by reiterations of successes and frustrations with little attention given to what might be underlying reasons for those intellectual and emotional highs and lows. I would like to propose that teachers be given opportunity and responsibility in the service of better understanding their schools and classrooms. Research on teaching procedures and findings can be a starting point in coming to such increased understanding. I urge teacher educators to move with teachers and teacher candidates toward examining their own settings and their own behaviors rigorously and persistently, using as stimuli what has come to be known about teaching and learning through research efforts. Examples of such teacher inquiry might

include teachers observing one another using well-developed coding systems to determine if their settings are reflective of what has been found in original studies, teachers banding together to discover and analyze the apparent relation between their schools and those described as "effective" in the research literature, teachers working together on verifying such concepts as ALT in their own schools and determining differences and similarities from the original BTES study.

In a related inquiry, (Tikunoff, Ward, and Griffin [1979]) it was reported that the benefits of teachers participating in systematic research included:

> . . . (1) awareness of options and possibilities, (2) acquisition of knowledge and skill related to r and d, (3) professional collegiality, (4) participants' self-perceptions of professional worth, (5) educational cosmopolitanism, (6) utilization of knowledge, (7) action upon school problems, (8) alteration of professional practice. . . (p. 407).

Although the consequences thus reported were ones associated with a relatively formal involvement in methodologically rigorous and conceptually detailed research procedures, the study gave evidence that teachers' professional growth was enhanced by the experience. I would like to test my belief that teacher education programs could be enhanced and participants' skills, beliefs, and knowledge altered positively as a result of attending to issues, procedures, and conclusions drawn from research on teaching and re-enacted in classrooms by teachers and teacher candidates.

The last of the three recommendations I have for the use of research on teaching studies for teacher education relates directly to the fourth tier of the simple paradigm presented earlier -- improvement. As many of us know and as I have indicated in this paper, the

logical leap from findings to efforts at change or improvement seems to have been fraught with difficulties and, at least from a purely intellectual point of view, lacking in success. I believe this state of affairs is a direct result of a narrow and unfortunately-conceived vision of how improvement or change takes place. Returning to the curriculum field for an example, many of us can recall the attempts to introduce curriculum and organizational changes in schools, some major and some minor. From new math through new biology to new social studies and from nongraded to multi-age grouping of students, there are those of us who worked willingly toward the end of promoting these innovations only to be told by Goodland and Klein (1974) that business as usual was taking place "behind the classroom door." What happened to these propositions for improvement is, I believe, happening today to the translation of teaching research findings into practice.

We didn't have the vehicle for promoting effective and lasting change. We had the content but we didn't have the delivery system to make that content present and meaningful to teachers and students.

We are in a better position today in that we have a decade or more of research findings regarding the change process. We have the landmark study from The Rand Corporation which has described improvement practices vividly and has been distributed widely (McLaughlin, 1976). We have at our disposal the findings from the five-year study of school change conducted by the Research Division of I/D/E/A (Bentzen, 1974). We have the beautifully conceptualized and elegantly actualized Concerns-Based Adoption Model (Hall and Loucks, 1978). We have, in short, a body of research evidence which suggests that certain considerations should guide

change and that certain activities can promote change.

In order for teacher educators to move the research on teaching findings into the improvement arena, it seems to me necessary that we consider guidelines from the change literature as principles for delivering the content derived from research on teaching studies. This, of course, calls for a broadening of our research vision to include two complicated and, it must be admitted, sometimes confounding bodies of research literature. If, however, our goals include the intention that teachers will be able to engage themselves in the role functions and features of the "effective" teacher, we must be able to conceive of ways to bring teachers to that level of performance. So far, there is little evidence that we can do this successfully as a consequence of edict, policy declaration, or entreaty.

It is my belief and hope that teacher education, in higher education settings and in ongoing and complex elementary and secondary schools, can best use the results of an increasingly sophisticated and productive body of work designed to understand teaching if teacher educators also understand and use the findings from inquiries into change as their means toward the end. With this effort, it can be speculated, the improvement of teaching and learning as an intention of teacher education has a much better chance for success than has been so far demonstrated.

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